LETTERS TO THE EDITOR.

**** *Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

The temperature of the moon.

The interesting article by Mr. Ferrel in your issue of Dec. 18 seems to require some words of comment for the general reader, who may not otherwise notice that the whole reflected heat (and light) of the moon appears to be there omitted from consideration. Moonlight is the ocular evidence that a part, at least, of the moon's heat is lost by reflection, since what is light to the eye is heat to the thermoscope. In fact, what we see is but about one-third of what is reflected; and all of this must be subtracted from what the sun sends the moon before we have as a remainder the amount radiated from its surface, which is treated as the whole sum in the article under discussion.

It is there assumed that the moon loses heat by radiation only; but, even in this hypothesis, the highest temperature assigned to its sunlit surface is but little above that of boiling water. Since, then, bodies only begin to be visible by radiation at a red heat, it follows from this hypothesis that the full moon would always be black and invisible, — an imaginary moon, and not the moon which we see. Mr. Ferrel is doubtless aware of this, and in his own view may be supposed to be purely treating of a hypothetical body; but the ordinary reader is certainly apt not to understand the strictly limited premises with which he starts.

In the exactness and competence of Mr. Ferrel's mathematical treatment of this or any subject he presents, all will agree; but the more exact the logical instrument, the more certain it is to deduce limited conclusions from limited premises.

Without entering on any discussion of Mr. Ferrel's use of Dulong and Petit's formula, I may then say that to those astronomers and physicists who are engaged in the task of experimentally determining the actual temperature of the lunar surface, the existence of this great amount of reflected heat is an enormous difficulty, for it is not until this has been differentiated from the radiated heat that the temperature of the actual surface is either theoretically or experimentally ascertainable.

To the present Earl of Rosse belongs the credit of making the first attempt to do this, and, in doing so, to conquer those experimental difficulties which lie even at the threshold, and which alone are excessive; for the total amount of all the heat of both kinds is so minute as not to change the reading of a thermometer directly exposed to the rays of the full moon by nearly so much as the thousandth part of a centigrade degree.

The writer has now been engaged for a long time in these researches, whose interest and importance to us are not to be measured by the minute amount of the heat in question.

To prevent mistake, let it be stated that there never has at any time been any doubt but that the lunar surface radiates heat toward us, and there is scarcely a doubt but that this radiated heat is greater than the reflected. The question is, however, as to the amounts, and as to whether the first kind passes through our atmosphere as well as the second.

This is not the place to discuss this somewhat recondite point; but the as yet unpublished Allegheny observations, now conducted through over twenty lunations with the object of discriminating the reflected from the radiated heat by the formation of a lunar heat-spectrum, show that a considerable part of this radiated heat does pass through our atmosphere along with that reflected. While the writer differs from the conclusions of Lord Rosse as to the temperature of the lunar surface, it seems due to truth to say, that, in the particular just alluded to, the interpretation of Lord Rosse is sustained more fully than his own first one.

Without anticipating the publication of these experiments, the reader may care to learn of one observation made on the rare occasion when the full moon is partially dark; that is, during an eclipse. In the lunar eclipse of Sept. 23, 1885, about eight-tenths of the moon's diameter was covered by the umbra. The night was beautifully clear at Allegheny, and observations were made with the bolometer on different parts of the lunar image formed by a concave mirror of twelve inches aperture, and ten feet three inches in focal length, which was kindly loaned for the occasion by Mr. J. A. Brashear. The image was a little over an inch in diameter, and the bolometer was limited by a diaphragm to an aperture of about three-tenths of an inch; so that any circular portion of the moon's surface forming about one-eleventh of the whole could be examined independently of the rest. Previous observers have been obliged to utilize all the lunar rays from a large concave mirror in forming a very small image barely covering the thermopile employed; but, owing to the superior delicacy of the bolometer, it has thus become possible to select small portions of a comparatively large lunar image for separate study, and still have heat enough for accurate measurement.

Before the eclipse began, the exposure of the bolometer to the central portion of the image produced a galvanometer deflection of one hundred and eighty divisions. The deflection on the east limb of the moon was one hundred and sixty-four divisions; but, as the eclipse advanced, the deflections here fell off very rapidly, the diminution being noticed before the penumbral shade became certainly visible to the eye. The diminution of the effect on the centre and west limb followed that on the east limb in time, as these regions were progressively covered by the shadow. On portions covered by the umbra the deflection was very small, varying from four divisions soon after the beginning of immersion, to scarcely more than a single division of the galvanometer scale shortly before emersion from the umbra; so that the deflection was with difficulty detected. This last minute effect might have been due to true radiation from the darkened lunar surface, or possibly to diffuse and irregularly reflected heat from the surface of the mirror, though the method of exposure was calculated to eliminate this source of error as far as possible, — a doubt which must be resolved by future experiment.

As the middle of the eclipse approached, measures made just outside the edge of the umbra indicated an increasing transmissibility by glass for the feeble radiant energy remaining. Thirty minutes before the middle of the eclipse, the transmission by glass for the lunar heat rays at this inner edge of the penumbra was found to be thirty-two per cent, and fifty-five minutes later it had increased to forty-eight per cent. Although these latter deflections were very small, the observations were apparently trust-

worthy. The average transmission of the lunar rays by glass during the eclipse was about twenty-two per cent, and did not differ very materially from that for the uneclipsed moon on this day. If the increased transmissibility at the outer edge of the umbra be a real effect, it is possibly local and evanescent.

The deflection obtained from a portion of the lunar surface just in advance of the umbra did not very materially differ from that given by a similar portion over which the umbra had just passed.

Clouds, preventing further observations, began to form as the penumbra was about passing off. There were indications, however, of a recovery of heat nearly as rapid as the previous fall. This effect was shown, though in a less marked manner, by Dr. Boeddicker's observations, in the eclipse of Oct. 4, 1884, made at Lord Rosse's observatory (see *Nature*, xxx. p. 589).

The following are the deflections observed on each point during the progress of the eclipse at Allegheny:

	Deflection.	Time.	Time from mid-eclipse.		Deflection.	Time.	Time from mid-eclipse.		Deflection.	Time.	Time from mid-eclipse
		h. m.	h. m.	Centre.		h. m.	h. m.	West		h. m.	h. m.
East	164	11.53	2.35	"	180	12.01	2.27	"	155	12.16	2 12
"	125	12.26	2.28	"	128	12.44	1.44	"	153	12.32	1.56
"	45	12.53	1.35	"	101	1.06	1.22	"	129	12.58	1.30
N.E.	4	1.28	1.00	"	31	3.49	1.21	"	21	4.05	1.37
S.E.	71	3.43	1.15								

The salient feature of these observations is, we need hardly say, the extraordinary rapidity with which the lunar surface parts with its heat, most of that which is radiated disappearing all but simultaneously with that reflected. S. P. LANGLEY.

Allegheny observatory, Dec. 23.

Sir William Thomson to the coefficients.

I know of no easier way to reach those for whom the enclosed message was especially intended than through the columns of *Science*. At the same time, I believe it will be read with great interest by many who were not of the somewhat limited number referred to. To such, a brief explanation may be due: —

At the close of the course of lectures by Sir William Thomson, at Baltimore, in October, 1884, it was determined by those who, through the courtesy of the Johns Hopkins university, had enjoyed the privilege of listening to the course, to present Sir William with a memento of the occasion which had been, to them, of such unusual interest. Under the circumstances, nothing could have been more fitting for this purpose than one of Professor Rowland's large concave gratings, which was accordingly agreed upon. Several months were required for the manufacture and examination of a grating which was entirely satisfactory to Professor Rowland; but early in the past summer it was completed, and transmitted to Sir William Thomson through the kindness of the secretary of the Smithsonian institution.

Prof. George Forbes of London was present during the course of lectures, and Lord Rayleigh attended a number of them. In the equations of motion developed in the work there appeared twenty-one coefficients, agreeing in number nearly, if not exactly, with the number of persons in regular attendance upon the lectures. This relation was quickly noticed by some one, and was made the basis of some humorous verses composed by the genial and witty Forbes, which were read at a reception given to the class by President Gilman, and were afterward published. Their title was "The lament of the twenty-one coefficients in parting from each other and from their much-esteemed molecule."

The first stanza began, —

"An aeolotropic molecule was looking at the view.
Surrounded by his coefficients, twenty one or two;"

and the whole will always possess much interest to those who were present. With this explanation, I justify the title which I have given to the following selections from a letter recently received from Sir William Thomson. T. C. M.

Washington, D.C., Dec. 28.

I wrote to Professor Rowland, acknowledging the receipt of the grating; but I ought before now to have thanked all the other coefficients for their kindness in giving it to me. I should feel greatly obliged if you would transmit to those of the coefficients who are in America my heartiest thanks for their great kindness, and say to them that the grating will be a permanent memorial to me of the happy three weeks of 1884, when we were together in Baltimore. . . . After the British association meeting at Aberdeen, I was delighted to be able to show the grating to some of our English appreciators, — including one of the coefficients, George Forbes; and Lord Rayleigh, whom we may consider as, at all events, a partial coefficient; and Professor Fitzgerald of Trinity college, Dublin; Oliver Lodge of Liverpool; Glazebrooke of Cambridge; and Captain Creak of the compass department of our admiralty, — who came to stay with us at Netherhall, our country house, for a few days, on their way south. We had no sunlight to work with, but we got the double sodium light in the first and second spectrums from a salted spirit-lamp flame exceedingly well, and we were all delighted with the result. I had never myself seen any thing like it before. WILLIAM THOMSON.

The university, Glasgow, Dec. 5.

A waste of public money.

My attention has just been drawn to your notice in *Science* of Dec. 4, of my forthcoming report on irrigation. The substance of your criticism is that quantity, and not quality, appears to have been the object in its compilation, — that the work should have been written in one volume instead of three; and you quote a long, redundant paragraph as a sample of the composition throughout.

It is to be regretted that you undertook to criticise an entire report, when you had before you only some advance sheets of one volume, very hastily printed from unrevised manuscript, solely for the purpose of an exhibit to the legislature, which desired to know something of the scope of the work.

The entire report, as ordered printed, is now under way; and I believe you will find, when you receive a copy, a decided improvement in the literary construction which you have criticised. As for the general make-up of the work, — its fulness, and occasional repetition of matter under different headings, — which you do not specially refer to, but probably have noticed, I shall have something to say at the